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myself very often obtained when the fixation had been imperfect. It is, of course, easy in these plants to secure admirable preparations of the stages preceding and following on the maiotic divisions, but I am sure Mr. MOORE will agree with me as to the great difficulty encountered in successfully fixing the cell contents at this critical period. Personally, I have not found chromacetic acid (the fixative used by him) very successful, but obtained far better results with Flemming's solution and, if due precautions are taken, with acetic alcohol. The latter, in particular, has yielded results of especial excellence, owing partly, no doubt, to the relative rapidity with which it traverses the somewhat impervious cell wall.—J. B. FARMER, *Royal College of Science, London*.

REPLY.

PROFESSOR FARMER acknowledges that in 1894 he believed in the simultaneous distribution of the chromosomes to the four spores in *Pallavicinia decipiens*. His description stands as the only account of a process without parallel in the plant kingdom, and he must have realized its exceptional nature. The account became all the more remarkable when Professor FARMER's own studies on a number of liverworts, published in the following year, showed two successive mitoses in the spore mother-cells as in other groups of plants. He acknowledges now that he may have missed the binucleate stage. This is precisely what I believe he did, but since I have not investigated *P. decipiens* I cannot assert that he did so. Now he states that this simultaneous distribution is really not the essential matter at all. Apparently the essential matter to him is his observation that several liverworts conform to the normal sequence of nuclear division during sporogenesis. Yet these conclusions, bearing as they do on *Pallavicinia decipiens*, served to emphasize the peculiarities of that account, and I feel confident that most, if not all, cytologists would pick out the description of a simultaneous distribution of chromosomes as the most essential feature of his paper of 1894.

I venture to think that botanists are not so much interested in the explanations which Professor FARMER may make of what he did or did not believe in 1894 and 1895 relative to the quadripolar spindle (which opinions they can form for themselves), as in the facts of sporogenesis in the liverworts. My study of *Pallavicinia Lyellii* is plainly a challenge of his account of *P. decipiens*, and together with Professor DAVIS's work on *Pellia*, leads us to believe that the "quadripolar spindle" in all liverworts is a phenomenon of prophase followed by spindles of two successive mitoses, in essential agreement with the events of sporogenesis in other plants.

The reader must judge for himself whether it is at all likely that two species in the same genus should differ from one another so fundamentally as would appear from Professor FARMER's description of sporogenesis in *Pallavicinia decipiens* and my own account of *P. Lyellii*.

Respecting the fixation of my material, I may say that I have no reason to think the penetration was not sufficiently rapid to fix the cell contents. Even with imperfectly fixed material my main conclusion is easily demonstrable, viz., that in *P. Lyellii* there are two successive mitoses in the spore mother-cell. Let us not lose sight of the main point at issue.—
ANDREW C. MOORE, *South Carolina College, Columbia.*